# Interdisciplinary research: Abiotic sinks for organic matter from Delta agricultural drains:Effects of coupled hydrodynamic and physicochemical processes

**James O Sickman**

## Public Comments

No public comments were received for this proposal.

# Collaboration Panel Review

## Proposal Title

#0171: Interdisciplinary research: Abiotic sinks for organic matter from Delta agricultural drains:Effects of coupled hydrodynamic and physicochemical processes

| Final Panel Rating |
| --- |
| adequate |

## Collaboration Panel (Primary) Review

### Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

**above average**
**complex series of experimental and field measurement activities conducted by four institutions linked together to produce creative answers to questions regarding transformations and fate of organic matter in Ag drains**

### Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

**superior**
**very clearly linked subprojects that will result in a clear integration of field and laboratory activities that will result in a conceptual model**

## Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

**above average**
**clearly defined task breakdown with clear management; timelines appear realistic**

## Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

**above average**
**there are clear commitments from team leaders; team members have complementary skills; the lead investigator, who has a record of successful collaboration experiences, has the challenge of managing a geographically diffused team.**

## Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

**adequate**
**no mention was made in the proposal--it is assumed that journal papers will be written and Calfed presentations will be made**

## Additional Comments:

**the distant locations of part of the team might present logistical problems, and could lead to some minor inefficiencies of time, communication, and resources**

# Collaboration Panel (Discussion) Review

**Primary reviewer judged the overall proposal as adequate; during the panel discussion, however, primary reviewer changed his rating for communication to inadequate because of the lack of explanation.**

**Secondary reviewer judged the proposal as adequate due to the poor description of how the results would be communicated.**

# Technical Synthesis Panel Review

## Proposal Title

#0171: Interdisciplinary research: Abiotic sinks for organic matter from Delta agricultural drains:Effects of coupled hydrodynamic and physicochemical processes

| Final Panel Rating |
| --- |
| **adequate** |

## Technical Synthesis Panel (Primary) Review

### TSP Primary Reviewer's Evaluation Summary And Rating:

**The proposed work focuses on the fate of Organic Matter that is in agricultural drain water entering the Delta, how it is transformed in such a way as to contribute to the pool of bioavailable carbon and possibly to disinfection byproducts in treated drinking water. The principal tools used include the observation of physical processes via optical and other methods (flocculation, coagulation, adsorption), and especially changes in carbon−14 content as these processes proceed during drainage of agricultural water into a selected river system in the Delta. Field and lab studies are proposed in an ambitious, multi−investigator project. Overall, the proposal appears to be well conceived and the exposition is adequate. The research plan is well done, the budget is reasonable for such a large project, and the team well qualified. There are some weaknesses in the proposal, however, which are noted in some detail by one of the external reviewers. Among the issues of concern are the following: 1) criticism that the study focuses on organic carbon from agricultural drains, which are known to be only ~7% of the total carbon entering the system; 2) the study depends upon carbon−14 aging measurements, which the reviewer contends may not give an accurate picture of the carbon content, given that small contributions of fossil−carbon can distort the**

**interpretation of these results; 3)therefore, as others have established, the team should be proposing to use several measures of organic carbon content and function; 4)the proposal reveals a lack of familiarity of the authors with a body of published work in this area; 5) this is especially apparent in the discussion and proposed work in the study of the role of photochemistry in the mineralization of OM. It was surprising not to see further studies proposed on the organic matter fractions, especially NMR and mass spec studies that can reveal changes in chemical composition, and measures of molecular/particle size.**

## Additional Comments:

**The proposed work focuses on the fate of Organic Matter that is in agricultural drain water entering the Delta, how it is transformed in such a way as to contribute to the pool of bioavailable carbon and possibly to disinfection byproducts in treated drinking water. The principal tools used include the observation of physical processes via optical and other methods (flocculation, coagulation, adsorption), and especially changes in carbon−14 content as these processes proceed during drainage of agricultural water into a selected river system in the Delta. Field and lab studies are proposed in an ambitious, multi−investigator project. Overall, the proposal appears to be well conceived and the exposition is adequate. The research plan is well done, the budget is reasonable for such a large project, and the team well qualified. There are some weaknesses in the proposal, however, which are noted in some detail by one of the external reviewers. Among the issues of concern are the following: 1) criticism that the study focuses on organic carbon from agricultural drains, which are known to be only ~7% of the total carbon entering the system; 2) the study depends upon carbon−14 aging measurements, which the reviewer contends may not give an accurate picture of the carbon content, given that small contributions of fossil−carbon can distort the interpretation of these results; 3)therefore, as others have established, the team should be proposing to use several measures of organic carbon content and function; 4)the**

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# Technical Synthesis Panel (Discussion) Review

## TSP Observations, Findings And Recommendations:

**The technical reviewers reached different conclusions regarding the technical merits of this proposal. The more critical review provided a number of detailed comments to substantiate the review's conclusions. The panel considered a number of these comments to be significant concerns. In particular, the team may not have adequate capability to perform all of the proposed research adequately, and some of the study's methodologies (heavy reliance of C−14 aging of OM and apparent inexperience with photochemical studies), as well as some of their assumptions (e.g., regarding transformations of biotic and abiotic carbon)may be problematic.**

# Technical Review #1

proposal title: Interdisciplinary research: Abiotic sinks for organic matter from Delta agricultural drains:Effects of coupled hydrodynamic and physicochemical processes

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

| | |
|---|---|
| **Comments** | **The goals, objectives and hypotheses are clearly stated, but I do not believe the ideas are novel or exciting. The hypotheses are not well founded (see additional comments).** |
| **Rating** | **good** |

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

| | |
|---|---|
| **Comments** | **A conceptual model is clearly stated and it directs much of the proposed research. However, the conceptual model is not supported by previous research. Based on existing information it can be concluded that the central hypothesis and conceptual model do not address a quantitatively significant fate for terrestrial OM in the Delta.** |
| **Rating** | **good** |

### Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

| | |
|---|---|
| **Comments** | **The proposed field and laboratory approaches are feasible and will address some of the objectives of the project. They will add to the base of existing knowledge, but they are unlikely to reveal novel information or advance fundamental understanding of OM processing. Some of the experimental approaches are highly manipulative and of less value for application to field conditions. Some of the information generated from this study could be useful for policy decisions.** |
| **Rating** | **good** |

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

| | |
|---|---|
| **Comments** | **The proposed research is ambitious, but it is technically feasible. The authors should be able to complete most of the proposed studies. My larger concerns are not the feasability of the study but the usefullness of the products.** |
| **Rating** | **very good** |

## Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

| | |
|---|---|
| **Comments** | **Monitoring is not a significant component of the proposal.** |
| **Rating** | **not applicable** |

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

| | |
|---|---|
| **Comments** | **It is difficult to see how the various rate measurements from experiments can be applied to processes occuring in the field. The study would mainly result in qualitative evaluations of the significance of specific processes in the fate of terrestrial OM in the Delta. However, it is doubtful that these qualitative evaluations would be more useful than evaluations derived from a carefull analysis of the Delta system and existing knowledge about the processes being investigated.** |
| **Rating** | **fair** |

## Additional Comments

| | |
|---|---|
| **Comments** | **Various aspects of the fate of terrestrial organic matter (OM) in the Sacramento–San Joaquin Delta would be investigated in this project. The project will specifically focus on OM in agricultural drainage, which contributes ~7% of the OM to the Delta. The roles of abiotic processes in the transformations and fates of terrestrial OM are considered to be of major importance in this system, because previous studies have indicated the OM is of limited bioavailability. The PIs have assembled a group of scientists that have experience working in the Delta and many have worked together on previous projects. This is a large project that includes field and laboratory components.**<br><br>**The transformations and fates of terrestrial OM have been important topics of research for several decades, and I was surprised by the authors' lack of familiarity with this large body of published research. It would be useful to have this background** |

**knowledge for shaping the proposed research. The proposed studies rely very heavily on the use of radiocarbon as a tracer of peat−derived OM, but none of the limitations of this approach are discussed. Natural organic matter, in dissolved, colloidal and particulate forms, is comprised of thousands of components that vary in composition and radiocarbon age. Therefore, radiocarbon ages of bulk OM are grand averages and there is no information about the distribution within this complex mixture. A small amount of "dead" OM, such as petroleum−derived components, will skew the bulk radiocarbon age of a mixture that contains mostly modern carbon. There is a 30+ year history of tracing OM in complex systems, such as the Delta, that have many sources of OM, and all of these studies conclude the need for the use of multiple tracers.**

**A major emphasis of the proposed research is to investigate the abiotic "short−circuit" of the microbial loop in the transfer of peat−derived carbon to higher trophic levels. The authors never provide fundamental arguments that explain why so much time and effort would be dedicated to a process that is of minor significance. Previous studies demonstrated that most of the terrestrial OM is in dissolved form and that this DOM is unavailable (**

**Photochemical transformations have been shown to play a major role in the mineralization of terrestrial OM, but this abiotic mechanism of removal is only briefly mentioned. Again the authors unfamiliarity with this topic is very apparent. The small amount of research proposed is inadequate to address the role of photochemical processes in OM transformations and removal. The export of terrestrial OM from the Delta is also likely to be a major abiotic mechanism for its removal, but this is not addressed in the proposal. Adsorption to mineral phases is discussed, but there are no studies to investigate the quantitative significance of this process or the fate of adsorbed**

| | |
|---|---|
| | **OM. Much of the proposed experimental work includes so many manipulations, such as filtration and addition of poisons, that it will be difficult to relate the information to processes in the field. How can the rates of specific processes in lab experiments be related to those in the field? This should have been addressed in the proposal.** |

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

| | |
|---|---|
| **Comments** | **Many of the authors have established good track records in reseach. In addition, the group has considerable expertise and the capability to conduct the proposed research. I was disappointed to see so few publications resulting from prior CalFed support. This should be addressed by the authors.** |
| **Rating** | **good** |

## Budget

Is the budget reasonable and adequate for the work proposed?

| | |
|---|---|
| **Comments** | **There is a lot of research proposed in this project. The budget reflects this work load and appears adequate.** |
| **Rating** | **very good** |

## Overall

Provide a brief explanation of your summary rating.

| | |
|---|---|
| **Comments** | **This summary rating primarily reflects my evaluation of the originality of the proposed research and the likelyhood that it would advance knowledge and thinking about the transformations and fates of** |

| | |
|---|---|
| | **terrestrial OM in aquatic environments. I don't thnk this project would address the major issues concerning the biogeochemistry of OM transformations. The greatest opportunities for developing effective management policies will likely come from breakthroughs in fundamental thinking and understanding of processes.** |
| **Rating** | **good** |

# Technical Review #2

proposal title: Interdisciplinary research: Abiotic sinks for organic matter from Delta agricultural drains:Effects of coupled hydrodynamic and physicochemical processes

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

| | |
|---|---|
| **Comments** | **Yes. Although this is a very complex proposal, the goals, objectives and hypotheses are never lost in the in the text. Because OM is important in supporting aquatic life, yet can cause problems with drinking water supplies as precursors to DBPs, it is essential that the fate and transport of OM is understood in this system that supports multiple uses. The concept that abiotic sinks have a significant influence on OM in the system is one that needs to be investigated.** |
| **Rating** | **excellent** |

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

| | |
|---|---|
| **Comments** | **Definitely. The lack of current understanding of how older, agriculturally−derived (peat) OM behaves (human−induced impacts) justifies this project due to the competing uses desired of the system by society. The proposal does a very good job of laying out its justification.** |
| **Rating** | **excellent** |

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

| | |
|---|---|
| **Comments** | **The approach is extremely well designed and thought out. The use of microcosm experiments, field measurements and experiments, and modeling should provide a comprehensive picture of the fate and transport of OM in this system. The use of isotope tracers and optical dyes in the field studies should provide excellent results. The knowledge gained from this project will allow managers of the system to make better−informed decisions in the future.** |
| **Rating** | **excellent** |

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

| | |
|---|---|
| **Comments** | **The proposal provides excellent documentation on their approach and appears to be well thought out. Although this is a very complex undertaking, it appears to be well within the capabilities of the investigators. The information gathered should be able to prove or disprove the stated hypotheses.** |
| **Rating** | **excellent** |

## Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

| | |
|---|---|
| **Comments** | **Their monitoring plan is well documented and combines both controlled microcosm experiments with pre/post** |

| | |
|---|---|
| | **field monitoring. It provides considerable detail and is well thought out.** |
| **Rating** | **excellent** |

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

| | |
|---|---|
| **Comments** | **Not only will the knowledge obtained be valuable to managers of the delta system, it will contribute to scientific understanding of OM fate and transport in general. This knowledge will be transferable to several other systems. The planned products target both the CALFED audience and a broader national scientific audience. Practical information derived from this project should be transferred to entities with land management responsibilities.** |
| **Rating** | **very good** |

## Additional Comments

| | |
|---|---|
| **Comments** | **This is probably the most comprehensive, well thought-out research project I have ever seen that will generate practical, real-world knowledge for decision makers. The questions that this project attempts to answer have parallels in other systems. I am aware of a similar situation in the middle Missouri River, where it has been suggested (but no research has been done) that declines in biological diversity may relate to the lack of suitable OM, yet communities are concerned about DBP formation in their drinking water drawn from the river. The knowledge gained by** |

| | |
|---|---|
| | **this proposal should be transferable and beneficial to other locations.** |

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

| | |
|---|---|
| **Comments** | **CVs of the team indicate a high degree of competence with this type of research along with considerable experience with the techniques that are proposed. The support infrastructure is assumed to lie within the two universities and other cooperators.** |
| **Rating** | **excellent** |

## Budget

Is the budget reasonable and adequate for the work proposed?

| | |
|---|---|
| **Comments** | **Yes, A lot of work is contained in the proposal; hence I feel the $1M+ budget is justified for the work that will be accomplished. All items I would expect were covered in line-item fashion. However, I have always been troubled by the high overheads that Universities need to charge. Is this negotiable?** |
| **Rating** | **very good** |

## Overall

Provide a brief explanation of your summary rating.

| | |
|---|---|
| **Comments** | **This is a very detailed and well-organized proposal. Every question that came to me during my first reading was answered by the proposal at some point. Obviously, a lot of forethought and planning went into its development.** |

Technical Review #2

| | |
|---|---|
| **Rating** | **excellent** |